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Rev. CHARLES PRITCHARD, President, in the Chair.

John Matheson, Esq., Glasgow;
Jabez Moden, Esq., Wellington Street, Gloucester;
Rev. D. W. Durnell, Welton, Northamptonshire; and
Dr. Dodgson, Cockermouth,

were balloted for and duly elected Fellows of the Society.

Investigations on Airy's Double-Image Micrometer.
By Prof. F. Kaiser, Director of the Observatory at Leyden.

The director of the Radcliffe Observatory, Oxford—the Rev. R. Main—has desired that I should present to the Royal Astronomical Society a brief account of my investigations of Airy's Double-Image Micrometer, and I feel honoured in satisfying that desire. These investigations form however only a small part of my investigations on micrometer measurements in general, which I can still by no means consider as closed, and the complete description of which would require a whole volume. Therefore I must limit myself to mentioning a few results, that may be of some value for those who possess and use Airy's Double-Image Micrometer, and I hope at least to

give hereby a proof of the high value which I attach to this important English invention.

Already in the year 1855 Mr. W. Simms made, at my request, a double-image micrometer of Airy's invention, with a position-circle, for the Observatory at Leyden. This instrument was destined to be added to my 6-inch refractor there, and to serve for the measurement of planets, for which the wire-micrometer appeared less appropriate. Though Mr. Simms has delivered the instrument for the moderate price of 16*l.* 16*s.* it is very beautifully executed, and the modification of its construction that appears to me desirable would not be possible without considerably increasing its price.

The Astronomer Royal himself has given, in the *Greenwich Observations*, in the *Memoirs of the Royal Astronomical Society*, vol. xv., and in the *Monthly Notices of the Royal Astronomical Society*, vol. x., a complete theory and description of his double-image micrometer, therefore I may consider the instrument as sufficiently known. From that theory and description it appears that, with the double-image micrometer, errors are to be feared arising from the following sources: —(1.) Periodical errors of the micrometer screw; (2.) Variability in the mutual distance of the threads of that screw; (3.) Distortion of the images. The periodical errors of the screw may be very different at small differences in the readings of the micrometer-head, and require therefore a particular inquiry. The errors arising from the sources (2) and (3) do not compensate themselves in brief periods, and their common amount may be determined by the same inquiry.

In the micrometer of Simms one half-lens is quite fixed, while only the other can be moved by the micrometer-screw. This construction makes it impossible to eliminate the periodical errors of the screw at the measurements themselves. If the fixed half-lens could only be moved so much as the amount of one revolution of the micrometer-screw, we could hereby, according to Bessel's theory, eliminate at each measurement the periodical errors of the screw, and this would be a great gain. If the fixed half-lens could be moved as much as the moveable one, the measurements could be extended to angles twice as large as can now be measured with the micrometer. Then it would however be necessary that the two half-lenses should be shown together, with relation to the fixed lenses. By this modification the price of the instrument would certainly be considerably raised, but it would be worth while.

I should require too great a space to describe here in what manner I have determined the periodical errors of the screw of Airy's micrometer, while the instrument is not adapted to this inquiry. Therefore I will only mention the results obtained, from which it appears that the periodical errors of the screw are very considerable, and very different in its different parts. The scale by which the revolutions of the

screw are counted is extended from 0 to 50. In the year 1863 I demonstrated, by many hundreds of measurements, the periodical errors of the screw, from 5 to 5 revolutions, between the readings of the scale 10 and 40. If the periodical error be expressed, as usual, by the periodical function:

$$\varphi(u) = a\cos u + b\sin u + c\cos 2u + d\sin 2u +$$

in which u signifies the reading of the micrometer-head, transformed into degrees, according to my inquiry, we have for the periodical errors of the screw in its different parts, the following values:—

Scale 10 $\varphi(u) =$	$-1''{\cdot}075\cos u$	$-0''{\cdot}578\sin u$	$-0''{\cdot}149\cos 2u$	$-0''{\cdot}022\sin 2u$
15	$-0''{\cdot}806$	$-0''{\cdot}934$	$-0''{\cdot}183$	$-0''{\cdot}025$
20	$-0''{\cdot}720$	$-0''{\cdot}975$	$-0''{\cdot}216$	$+0''{\cdot}046$
25	$-0''{\cdot}721$	$-1''{\cdot}283$	$-0''{\cdot}146$	$+0''{\cdot}051$
30	$-0''{\cdot}655$	$-1''{\cdot}338$	$-0''{\cdot}142$	$+0''{\cdot}068$
35	$-0''{\cdot}835$	$-1''{\cdot}527$	$-0''{\cdot}140$	$+0''{\cdot}135$
40	$-0''{\cdot}729$	$-1''{\cdot}711$	$-0''{\cdot}310$	$+0''{\cdot}195$

It appears, therefore, that the errors of the screw are very variable, and, in themselves, five times larger than those of Bessel's heliometer.* The value of one revolution is, however, for the heliometer $52''$ and for Airy's micrometer, according to the different powers, from $5''$ to $11''$, so that the influence of these errors is from 5 to 10 times smaller than with the heliometer. The errors are however far too great to be neglected.

The common amount of the errors (2) and (3) is obtained by determining the value of the revolution of the screw for different values of the measured angle, or, which is the same thing, for different distances of the half-lenses. This amount will be imperceptible if, for all the distances of the half-lenses, there is found the same value of the revolutions of the screw, and then the movement of the half-lens will be sensibly proportional to the size of the objects that are to be measured. It is however extremely difficult to determine, with the required accuracy, the value of the revolutions of the screw. Even though Bessel had not positively declared it,† we could easily be convinced, by our own experience, that no observation of transits, or angle-measurement with a circular measuring instrument, can

* Bessel, *Astronomische Untersuchungen*, vol. i. p. 83.

† Bessel, *Astronomische Nachrichten*, No. 403, and *Astron. Untersuchungen*, vol. i. pp. 51 and 132.

give a micrometrical accuracy. Bessel preferred a heliometer, even for measuring small angles, because with that instrument it is possible to embrace also angles of more than one degree. If the value of the revolutions of the screw were determined by the ordinary means for a space of more than one degree, the inevitable error committed in this would be considerably diminished in angles of a few minutes, which are peculiarly to be measured by the instrument. With Airy's micrometer it is hardly possible to exceed the small angles that are to be measured therewith, and the errors committed in determining the revolutions of the screw are therefore not diminished in the results of the measurements which are to be performed with the instrument. I had requested therefore that Airy's micrometer, that is, a terrestrial eye-piece, should be constructed in such a manner that it might be directed on the wires of the wire-micrometer. The distance of the wires can be determined with the wire-micrometer free from all errors of the instrument. If that distance be measured again with Airy's micrometer, we might, with a micrometrical accuracy, derive therefrom the value of the revolutions of the screw, and Airy's micrometer would give the same accuracy as the wire-micrometer, in those cases in which the latter instrument, on account of the diffraction of light, does not allow a sharp definition. Unfortunately Airy's micrometer could only be adapted to the wire-micrometer with its two lowest powers, which are too small to allow any sharp measurements.

Airy's micrometer has four first lenses, with focal distances of $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, and 1 inch, which give, with the 6-inch refractor, magnifying powers of 326, 220, 143, and 109 times. Already in the year 1856 the value of the revolutions of the screw was determined for each of those powers, and for very different values of the measured angle. For doing this, with the two highest powers, the wire-micrometer was screwed on a refractor of Steinheil having an aperture of 4 inches and a focal distance of 9 feet, while Airy's micrometer was adapted to the 6-inch refractor. The object-glasses of both the refractors were directed on each other, and with Airy's micrometer the image of the wires of the wire-micrometer formed by both the object-glasses was measured. For the two lowest powers the micrometer of Airy was immediately adapted to the wire-micrometer. Thus the value of the revolutions of the screw was determined, with great care, for the different powers and for different measured angles, and I gave an extensive report of these investigations, with a number of remarks on Airy's micrometer, in an ample treatise that was, in the year 1857, published by the Royal Academy of Sciences at Amsterdam.*

After the foundation of the new Observatory at Leyden, in

* The title of that treatise, *Eerste onderzoekingen met den Mikrometer van Airy, volbragt door F. Kaiser.* Amsterdam: C. G. van der Post. 1857.

the year 1861, the 6-inch refractor was exclusively destined to extra-meridional observations of planets and comets, and the new 7-inch refractor of Merz was destined to micrometer measurements. Airy's micrometer was adapted to the 7-inch refractor, and, in the year 1863, I undertook a new investigation of Airy's micrometer, in quite a different manner from the former, in order not to be obliged to dismount the instrument for it. To the edifice of the University, at a distance of 240 metres from the refractor, I fixed a heavy black shelf, to which were attached some silvered copper disks. Two of these disks were very small, and were placed at a mutual distance of about 0·50 metres. The remaining disks had diameters of about 0·01, 0·02, 0·03, 0·04, 0·05, and 0·06 metres. The linear dimensions of these diameters, and of the distances of the small disks, were accurately measured, and this made known the ratio between these quantities. The angular value of the distance of the small disks was afterwards determined by the wire-micrometer, and, by the said ratio, from this, by proportion, the angular value of the diameters of the larger disks was derived. The angular distance of the smaller disks, amounting to above 7′, could be determined with a certitude of about 0″·1; and, as the diameters of the disks were from 8 to 50 times smaller, the angular values of these diameters became known with a certitude of 0″·01 or 0″·02. The diameters of the disks were measured again with Airy's micrometer, and so the values of the revolutions of the screw were determined for different distances of the half-lenses. When however I had measured two of these disks, with all the powers of the micrometer eye-piece, and each of them 50 times, my shelf with disks disappeared, without my being able to discover who had taken it away, and my investigation was broken off.

In the past year I had new disks made that were fixed to a piece of iron, and were attached to a part of the edifice of the University, where they were more safe than before. The measurements were repeated in the former manner, and extended with Airy's micrometer, on five disks, with diameters of 8″ to 45″. The result for each disk and each power rested on no less than a hundred pointings and readings. The silvered disks however soon gathered moisture, and they were soon so little brighter than the blackened iron that the measurements were extremely difficult.

Lately I effected quite a new investigation with respect to Airy's micrometer, in which I superseded the former disks by artificial double-stars. To the above-mentioned iron were screwed copper-plates with little apertures that appeared, by the refractor, under an angle of 0″·8, and at such distances as that they represented double-stars at distances of 7″·5 to 74″·6. Behind the iron was placed a mirror that reflects the daylight through the apertures towards the refractor, and by which these apertures show themselves very clearly with a clouded

sky. After these artificial double-stars were measured lineally, with great care, I measured each of these objects 50 times with the second power of Airy's micrometer.

All the above-mentioned inquiries have, without an exception, led to the result that the value of the revolution of the screw of Airy's micrometer increases with the measured angle, and that therefore the movement of the half-lens is not proportional to the distances of the images, and that a considerable error may be committed by assuming a constant value for the revolution of the screw. I cannot communicate my thousands of measurements for the investigation concerning Airy's micrometer with any completeness without requiring a space that I cannot dispose of here, but, as a proof, I communicate the measurements performed with Airy's micrometer, in the last year, with the second power, on eight artificial double-stars. Each of these objects which I shall name *a*, *b*, *c*, *d*, *e*, *f*, *g*, and *h*, was each day measured five times, and these measurements were repeated on ten different days, so that each result rests on a hundred readings. The results obtained each day, and expressed in revolutions of the screw, are the following:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
0·9862	2·0133	3·0023	3·9995	5·2918	6·2845	7·2445	9·7666
0·9924	2·0084	2·9943	3·9805	5·3083	6·2898	7·2382	9·7657
1·0019	2·0009	3·0067	3·9909	5·3026	6·2957	7·2611	9·7792
0·9954	2·0055	3·0002	3·9858	5·2856	6·2948	7·2448	9·7658
0·9891	1·9967	3·0148	4·0083	5·3001	6·2988	7·2673	9·7667
0·9975	1·9993	3·0072	3·9887	5·3188	6·2889	7·2745	9·7715
0·9977	2·0065	3·0065	3·9932	5·3011	6·2944	7·2709	9·7810
1·0041	2·0088	3·0192	4·0098	5·2993	6·3089	7·2810	9·7907
1·0068	2·0131	3·0113	4·0044	5·3098	6·2974	7·2626	9·7015
1·0149	2·0281	3·0207	4·0048	5·3248	6·2978	7·2630	9·7873
Mean 0·9986	2·0081	3·0083	3·9966	5·3042	6·2951	7·2608	9·7756

Airy's micrometer gives, with the 7-inch refractor, powers of 413, 278, 178, and 137 times. For the above-mentioned measurements the second power, namely that of 278, was used. If it be remarked that, with that power, the value of a revolution of the screw amounts to about 7″·5, it immediately appears from the above measurements that Airy's micrometer admits of a great accuracy, and therefore deserves a rigorous investigation. The above results require however still a small correction that arises from the manner in which I performed the measurements. If we call the images formed by the one half-lens *m* and *n*, and those formed by the other *m′* and *n′*, *n*

and m' were brought, as nearly as possible, under each other, and placed so that the line passing through their centres stood perpendicularly to the line that connects m and n' together. This was done by the moveable half-lens on the left-hand; by the moveable half-lens on the right-hand the same thing was done for m and n'. Of course the micrometer-screw was always turned in the same direction. Half the difference of the readings would have been precisely the distance of the centres if the images fell into the same straight line, but now it is a little too small. I chose this manner of measuring because it appeared to me it allowed a very sharp definition, and excluded the possibility of constant error. After applying the necessary small corrections, I found, for the true distances in revolutions of the screw, the following final results:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
1·0043	2·0109	3·0101	3·9980	5·3053	9·2959	7·2616	9·7762

With a comparator that I had composed from parts of other instruments, I repeatedly measured the linear distances of the different artificial double stars, and I got for this the following results, expressed in millimeters:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
8·625	17·455	26·253	34·915	46·365	55·195	63·820	86·290

The distance of the two most distant small apertures was 480·038 millimeters, and by often repeated measurements with the wire-micrometer in which the periodical errors of the screw were eliminated, it was found that this distance amounted to 20·0213 revolutions of the screw of the wire-micrometer. The value of each revolution of that screw had, by very numerous passages of stars, while the wires were 13′ distant from each other, been determined to be 20″·715, and from this ensued, by proportion, for the distances of the artificial double-stars, in seconds:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
7″·452	15″·080	22″·681	30″·165	40″·057	47″·686	55″·138	74″·550

From these numbers we find, with the second power of the micrometer, the following results for the value of each revolution of the screw. The number above each result signifies the distance of the half-lenses, expressed in revolutions of the screw, corresponding to the revolution which has been found:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
1·00	2·01	3·01	4·00	5·31	6·30	7·26	9·78
7″·420	7″·499	7″·535	7″·545	7″·550	7″·574	7″·593	7″·626

From this it appears therefore, that the value of the revolutions of the screw increases very sensibly with the size of the measured angle, and all the investigations of the same nature, performed with the micrometer, have led to the same result.

Constant errors in the measurements might cause different results to be found for the value of the revolutions of the screw at different angles, even if the micrometer were perfect. Thus, for instance, assuming that *a* and *h* have both been measured 3·01 revolutions, or 0″·23 too large, the same result, namely, 7″·649, would be found, but in the actual measurements it was totally impossible to commit a constant error of that amount. The measurements on the remaining artificial double stars are also contrary to the supposition of a constant error. If, namely, we assume the value of 7″·649 for all the artificial double stars, the errors of the measurements should have the following amounts :—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
− 0″·23	− 0″·30	− 0″·34	− 0″·42	− 0″·52	− 0″·47	− 0″·41	− 0″·23

As well the amount as the regular increase and diminution of these differences renders the supposition absurd that the value of the revolutions of the screw was constant. Although I was convinced that the errors of the above results could at the utmost amount to a few hundredth parts of a second, I have measured four of the artificial double stars — namely, *a*, *b*, *c*, and *d* — once more in a quite different manner. If we again name *m* and *n* the two objects of the one image, *m′* and *n′* those of the other; I now brought all the objects into the same straight line and placed, with movable half-lens on the left hand, the object *m′* first on the left and then on the right of *n*, so that there remained each time a black space as a *minimum visibile*. Afterwards the same was done by movable half-lens on the right hand with the objects *m* and *n′*, and the distance of the centres was thus obtained by four measurements instead of by two. I repeated the measurement each day five times, and performed this on eight different days. The results obtained on those days are the following :—

a	*b*	*c*	*d*
1·0060	2·0165	3·0199	4·0042
1·0030	2·0132	3·0147	4·0081
0·9959	2·0181	3·0212	4·0045

a	*b*	*c*	*d*
0·9979	2·0095	3·0175	4·0077
1·0057	2·0156	3·0179	4·0007
1·0034	2·0153	3·0156	4·0070
0·9961	2·0106	3·0183	4·0045
1·0062	2·0134	3·0153	4·0066
Mean 1·0018	2·0140	3·0175	4·0054

The absolute differences compared with the former results are for—

a	*b*	*c*	*d*
+ 0″·02	− 0″·02	− 0″·05	− 0″·05

Although the differences are greater than I expected, they are small enough to exclude the possibility of errors that amount to 0″·52. I prefer the first method of measuring, as it appears to me to allow the greatest accuracy in pointing.

If the value of a revolution of the screw is assumed to undergo a change proportional to the size of the measured angle; if x is named the value of a revolution of the screw for a distance equal to zero, and y the increment of that value for each revolution of the screw, each of the artificial double-stars gives an equation of the form—

$$m = n\,x + n^2\,y.$$

The solution of the eight equations obtained in this manner, according to the method of least squares, gave—

$$x = 7''\cdot4791$$
$$y = +\ 0''\cdot01511$$

and therefore, if r signifies the value of each revolution of the screw, and R the dimension of the measured angle in revolutions of the screw—

$$r = 7''\cdot4791 + 0''\cdot01511\ \mathrm{R}.$$

If we calculate by this the value of r for the different artificial double-stars, we get for—

	a	*b*	*c*	*d*
Observed ..	7″·420	7″·499	7″·535	7″·545
Calculated ..	7·494	7·509	7·525	7·539
Difference ..	+0·074	+0·010	−0·010	−0·006
Absolute error	−0·07	−0·03	+0·03	+0·02

	e	*f*	*g*	*h*
Observed ..	7″·550	7″·574	7″·593	7″·626
Calculated ..	7·559	7·574	7·589	7·624
Difference ..	+0·009	0·000	−0·004	−0·002
Absolute error	−0·05	0·00	+0·03	+0·02

The amount of the absolute error at *a* seems to indicate that the value of *r* increases more slowly in proportion as R grows larger. This deserves attention, since the second series of measurements also agrees with it.

In order not to make this paper too extensive, I will only mention, as regards my remaining investigations on Airy's micrometer, the final results of the measurements on five metallic disks performed in the year 1865. By very numerous measurements the diameters of these disks were found thus:—

	1	2	3	4	5
In Millimeters	9·987	20·020	30·047	40·027	45·928
In Seconds	8″·628	17″·296	25″·959	34″·581	43″·135

The diameters of these disks were, with the powers 1, 2, and 3 of Airy's micrometer, measured on each day five times, and these measurements were repeated on ten different days. The final results, expressed in revolutions of the screw, are the following:—

	1	2	3	4	5
Power 1	1·7407	3·4496	5·1267	6·8082	8·4488
Power 2	1·1535	2·3124	3·4531	4·5898	5·7117
Power 3	0·7596	1·4983	2·2341	2·9703	3·6930

From this follows for the value of *r*:—

	1	2	3	4	5
Power 1	4″·957	5″·014	5″·063	5″·079	5″·105
Power 2	7·480	7·480	7·518	7·534	7·552
Power 3	11·359	11·544	11·620	11·642	11·680

Consistently with all the investigations performed, we

see here again r increasing when the measured angle grows larger.

If we assume for the second power the before-found value of r, $r = 7''{\cdot}4791 + 0''{\cdot}01511$ R, we have for r at the different disks of which the diameters have been measured —

	1	2	3	4	5
Observed	7″·480	7″·480	7″·518	7″·534	7″·552
Calculated	7·496	7·514	7·531	7·548	7·565
Difference	+0·016	+0·034	+0·013	+0·014	+0·013
Absolute Error	−0·02	−0·08	−0·04	−0·06	−0·07

It appears therefore that the disks have been measured rather too large, which cannot be astonishing, since by the interference of light they must show themselves rather larger than they are. The result that Airy and Liouville have obtained by measurement, that the apparent diameters of disks are, contrary to theory, independent of the apertures of the refractors, deserves indeed a further inquiry. The mean of the absolute errors is $-0''{\cdot}056$. If we assume this amount, the remaining errors are, with

1	2	3	4	5
−0″·04	+0″·02	=0″·01	+0″·01	+0″·02

The measurements on the disks were sensibly less sure than those on the artificial double-stars, because the former very soon lost their whiteness. The disk 1 especially was generally very faint. I intend to repeat this inquiry with round apertures through which the daylight shines. It will hardly be necessary to remark that all the measurements recorded here are freed from the periodical errors of the screw.

It is easy, by a repetition of observations, to give to a final result of micrometer-measurement, a probable error of only a few hundredth parts of a second, but that probable error is a false measure for judging of the real error. It is ordinarily seen that final results for the same dimension, obtained by micrometer-measurements, differ some tenth-parts of a second from one another; and if it is considered from what complicated operations the above results have been derived, we have reason to be very well satisfied with their mutual harmony. An object with a diameter of one second presents itself, even with a power of 300, still so small to the eye, and the air is, moreover, continually so impure and unquiet, that we must despair of performing micrometer-measurements with a certainty of a few hundredth parts of a second, and I do not believe that this accuracy has ever been obtained, however small the probable error of the final result may be.

From the above-mentioned investigations it appears that the Astronomer Royal has, by the invention of his double-image micrometer, rendered an important service to Astronomy. I doubt very much whether, besides the Heliometer, a second double-image micrometer exists by which measurements can be effected so accurately as by Airy's double-image micrometer. That instrument requires however a very rigorous and difficult inquiry in order to give it the accuracy of which it is capable, and it requires also great prudence in the use of it. In my treatise of the year 1857 I have treated circumstantially of the precautions that Airy's micrometer requires, and of its properties.

A micrometer like Airy's can only render its services if it is adapted to a large and precious refractor. The price of the micrometer would, even if it were doubled, still remain very insignificant in comparison with that of the refractor. Therefore I should think it very desirable to give to the micrometer the above-mentioned more complicated construction, even if its price were thereby considerably increased. It would be very important if the periodical errors of the screw could be eliminated by the observations themselves, and, by a mobility of the fixed half-lens, the measurements could be extended to angles twice as large as those which the micrometer is now capable of measuring.

Leyden, Feb. 1866.

Additions to the Investigations on Cometary Systems.

By M. Hoek.

In § 11 of my paper "On the Comets of 1677 and 1683, 1860 III., 1863 I., and 1863 VI." present volume, pp. 1–12, I announced the necessity of a new combination of our whole stock of cometary aphelia with adequate enlargement of the limit of time. I have effected this investigation.

In order to have before the eye the whole of the phenomena furnished by the distribution of cometary aphelia and planes of orbits, I have made a drawing containing 190 orbits of comets discovered since the year 1556.* First, I laid down in it the aphelia whose positions were obtained by calculation after the elements that seemed to be the most reliable. Further I traced through these aphelia the circles which re-

* Periodical comets and elliptical as well as uncertain orbits have been excluded.

present in a stereographic drawing the intersections of the planes of the orbits with the sphere. But, in order to avoid a confusion which would have been intolerable, I limited these circles to a length of 10° in average on both sides of the aphelion.

It was the intention to find out those points of the sphere in which occurred an intersection of several orbits, the aphelia of which were distributed around these intersectional points. For, having discovered such points, we have reasons to presume that they indicate, each of them, a centre of cometary emanations, such as the point

I. ..... $\lambda = 319°$ $\beta = -78°{\cdot}5$ Mean Equinox of 1864·0.

the discussion of which was given in my former papers.

I find that it is possible that there are such centres in the following positions:—

II.	$\lambda = 267°{\cdot}0$	$\beta = -51°{\cdot}6$	Mean Equinox of 1864·0
III.	$\lambda = 175{\cdot}5$	$\beta = -46{\cdot}5$	
IV.	$\lambda = 75{\cdot}5$	$\beta = -51{\cdot}7$	
V.	$\lambda = 274{\cdot}6$	$\beta = +38{\cdot}7$	
VI.	$\lambda = 92{\cdot}9$	$\beta = +0{\cdot}6$	

and I proceed to indicate what there is remarkable in each of them.

II. is the intersectional point common to the orbits of the comets of 1739, 1793 II., 1810 and 1863 V. Unfortunately for our purpose, there is uncertainty with respect to one of these comets. It is possible, and even somewhat probable, that the Comets of 1810 and 1863 V. are two apparitions of a same body revolving every 53·3 years.

III. is the centre of a small circle of 1° in diameter, through which pass the orbits of the Comets of 1764, 1774, 1787, and 1840 III.

IV. is the centre of a small circle of 1° in diameter, through which pass the orbits of six comets, those of 1596, 1781 I., 1790 III., 1825 I., 1843 II., and 1863 III. At a small distance from this circle pass the orbits of the Comets of 1785 II., 1818 II., and 1845 III. But what gives to its centre a peculiar interest is that it is only 1°·5 distant on the sphere from the point

$\lambda = 73°{\cdot}2$ $\beta = -51°{\cdot}6$ Mean Equinox of 1864·0

in which occurs the intersection of the orbits of the Comets 1857 III. and 1857 V. As for me, I do not doubt that these two comets are members of a former system, when I

consider how small the interval of time was between their apparitions, and how uncommonly great the resemblance is in all the elements of their orbits.

It is true that among the eleven orbits, which may be connected with the point IV., there are some whose aphelia are distant from it by more than 10°, the average distance I had adopted. But we must remember, first, that there is something arbitrary in this limit; secondly, that we ought not to consider as exactly known the aphelia such as they are given by the computation of the orbits. Let us remember that often it is possible to represent the observations of a comet in different manners, by varying the longitude of the perihelion, and according to it the time of its passage. Further, that very often, when the comet is still at a great distance from us, the observations are rendered less certain by the circumstance that no nucleus is visible. Finally, that generally when the comet has approached our earth, and when we perceive its nucleus, its movement must necessarily be modified by the reaction it has to suffer from the particles evaporating under the influence of the Sun.*

V. is the centre of a circle of 3° in diameter traversed by the orbits of the Comets of 1773, 1808 I., 1826 II., and 1350 II. Unfortunately the orbit of the second of these is less certain.

VI. is the centre of a small circle of 52 minutes of angle in diameter, within which are accumulated the aphelia of the Comets of 1689, 1698, 1822 IV., and 1850 I.

The probability, *à priori*, of this phenomena being

$$(\sin^2 . 13')^3 \text{ or } 0,000\,000\,000\,000\,002925,$$

we could have expected its occurrence by chance only once in 341900 000 000 000 cases, and all our comets (190) furnish only 52590000 quaternary combinations, that is a $\frac{1}{6500000}$ part of the required number.

It is very unfortunate, indeed, to have here again a reason to doubt. The orbit of the Comet 1689 is rather uncertain, and the allowable variations of the elements would easily remove the aphelion far from the indicated limit of position.

Let us, however, observe that even when the orbit 1689 is wholly rejected, the point VI. retains a deal of its interest.

* It appears even probable to me that we must admit such influences in the aphelia of the Comets 1618 II., 1723, 1798 II., 1811 II., and 1849 I., all lying around the point

VII. ..... $\lambda = 217°.8$ $\beta = +26°.6$ Mean Equinox 1864.0;

in which four of these orbits meet, and which is only distant from the fifth (the orbit of the Comet of 1723) by 2°.1.

In order to show this, I have repeated the calculation, admitting that only three aphelia are contained within the circle.

The probability of that phenomenon is $(\sin^2 13')^2$ or 0, 000 000 000 2046, and therefore we may expect to find it once in 4889 000 000 000 ternary combinations. Now, that number of cases would only be furnished by more than 3050 comets, that is to say, sixteen times the number which actually has entered into my calculations.

I believe that it is sufficient to have called the attention of astronomers to these points of the sphere. Generally, there is too much doubt about the elements as well as about the periodicity of ancient comets. I do not wish to give here any decisive conclusions, and I expect the confirmation of my views from the future apparitions of such bodies. But, therefore, let me repeat it, it is necessary that their discovery be as much as possible assured.

A single remark finally. My drawing of aphelia shows a very remarkable phenomenon. Let us draw a circle through the points of the sphere

$$\lambda = 95^\circ \qquad \beta = 0^\circ$$
$$\lambda = 169 \qquad \beta = 32$$
$$\lambda = 243 \qquad \beta = 0$$

I say that the sector contained between this circle and the ecliptic is uncommonly poor in aphelia. Indeed, instead of containing fifteen of them, which would be required by a uniform distribution, it only contains one, that of the Comet of 1585, situated at 3° distance from the ecliptic.

How are we to explain this? If we knew that the solar system was removing from the point situated in the middle of that sector, I should be inclined to attribute the phenomenon to a difficulty Comets might experience in overtaking the Sun. But the direction of the solar motion, such as it was given by Mädler's investigations, does not allow of such an explanation. Therefore we may ask if the phenomenon is a real one, and there is in that direction of the heavens a scarcity of centres of cometary emanations; or rather, if it depends on the circumstances under which comets are ordinarily detected, the sector in question being so near the part of the ecliptic occupied by the Sun from July to December.

Utrecht, December 4, 1865.

M. Hoek has communicated the following corrections to his former papers:—

In the first paper, vol. xxv. pp. 243–251, § 2, the formula

$$\cos \delta = \frac{1}{1+a}\left(\frac{\rho}{r} + 1\right) \textit{ should be } \cos \delta = \frac{1}{1+a}\left(\frac{\rho}{r} - 1\right)$$

In the second paper, present volume, pp. 1–12, § 1, note to the Comets 1672, 1677, and 1683, 1677 is a stranger, *should be* 1672 is a stranger.

§ 2, belonging to the cometary systems of 1860 and 1863, *should be* cometary system.

§ 6, the equations quoted from the *Theoria Motus Corporum Cœlestium*, *v* should be *r*.

Same §, p. 9, line 21 from the bottom, to be reduced to $\frac{1}{8}$th part, *should be* $\frac{1}{6}$th part.

Same §, p. 9, line 6 from the bottom, repetition of perihelia, *should be* repartition.

Notice of the Great Nebula in Orion. With a Drawing.* By the Rev. T. W. Webb.

A comparison of the various representations of the Great Nebula in *Orion*, which have been given for a period of many years, seems to lead inevitably to the conclusion that our knowledge of its real aspect is still far from complete. While Mr. Huggins' most important discovery of its true constitution renders the idea of change more conceivable, it must be admitted that any alterations of form or brightness which may have been in progress since the employment of the telescope are so masked by discrepancies of delineation as to preclude the possibility of drawing any fully satisfactory conclusion. It would be superfluous to refer to the variations which are so well known to arise from diversity of weather and instruments, and eyes and pencils, and especially from the unwarrantable carelessness of engravers: it only remains to be considered whether these are the sole causes of the striking differences by which we are confronted; or whether, after making due allowance for all these sources of error, there may yet be a residuum of actual physical change. No other means of investigating this point seem so promising as the multiplication of designs by different observers, in different climates, and with different optical means. From such an accumulation of testimony we might hope to deduce a much closer approximation to the truth.

But if I have, from this impression, been led to attempt a very unpretending sketch, and to beg permission to lay it before the Society, I trust it will be understood that I am so sensible of its deficiencies that I should not have ventured to bring it forward in any other form than that of a suggestion,

* The drawing was exhibited at the Meeting.—Ed.

and with the hope that while there are so many instruments, on all sides, of greater optical power than my own, this reference to so interesting a subject, may induce some of their possessors to outdo, as they easily may, all that I have attempted here.

The sketch, of which a copy is now offered, was commenced in December 1863, and continued in a very desultory manner and with many interruptions till the present season, when, as the design was carried on into fresh regions, the detection of one or two features which do not appear distinctly in previous delineations, determined me to bring it forward at once, though merely as a temporary and provisional effort, to be perhaps corrected some future day by myself, and certainly improved by others. Had the weather not been so very unpropitious, I believe the region N. of the trapezium might have had a chance of being better represented; unfortunately, this is the very part to which I should wish especial attention to be directed; but I have been unwilling to incur further delay, as the present season for examination will so shortly be passing from us.

In this rough attempt to show some part of what may be visible in a certain circumscribed portion of the nebulosity, with an achromatic of $5\frac{1}{2}$ inches aperture, and an eye of average capacity, the object has been to represent the general arrangement of the luminous haze; and the few stars contained in the sketch are inserted merely for the purpose of more convenient identification and reference.

The three or four distinct masses of light S. of the trapezium were drawn at the end of 1863 and during the ensuing spring.

1864, Dec. 27. I suspected the existence of a narrow dark channel connecting the inner end of the *Sinus Gentilii* with a dark opening lying further on in the same direction, which does not appear in the design of Sir John Herschel, but is found in that of Bond.

1866, Jan. 5. Though the constellation had not risen more than half way to the meridian, and definition was fluttering, I perceived with powers of 111 and 212 a dark rift, of irregular breadth, lying on the whole nearly in a straight line a little S. of the stars 87 and 70 (those represented in the sketch). At the E. end it seemed to communicate with the *Sinus Magnus;* at its opposite extremity it opened into a dusky spot or lake, forming its darkest portion. It appeared broader S.W. of the star 87 than S.W. of 70. About half-way between 65 (the P. star of the trapezium) and 87, but a little in front of the joining line, there is a bright knot, at times seeming to inclose a minute star.

1866, Jan. 11. Though near the meridian, I did not see the dark rift so well as on Jan. 5. I could, however, distinctly make out the "lake" at its W. extremity. Its N. edge in

passing stars 87 and 70 seems a continuation of the *Sinus Magnus*, and the rift extends probably right through the luminous region W. of the lake. I do not think the projecting end of the *Regio Huygeniana* to the S. quite so conspicuous, as compared with the masses lying N.E. and N.W. of it, as when I sketched it in 1863 and 1864.

1866, January 25. The rift could still be detected, especially by averted vision, notwithstanding a moon two days past quadrature It seems to be feebly traceable beyond the "lake" to the W. into the dark opening which continues the direction of the *Sinus Gentilii* in Bond's drawing, so as to establish an uninterrupted, though in part very faint, demarcation round two sides of the brightest portion of the nebula, reaching from the mouth of the *Sinus Gentilii* to the bottom of the *Sinus Magnus*. With 450 the rift is still pretty distinct; in the triangle formed by the centre of the new lake, and the stars 67 and 70, the F. side is longer than the P., and the N. the shortest of all.

1866, Feb. 3. The sketch was completed for the present.

I have the less hesitation in bringing forward these particulars as I have the pleasure of knowing that my observations on this rift and the "lake" or opening have been in great measure confirmed by Mr. Knott with his beautiful $7\frac{1}{2}$-inch object-glass; and I, therefore, believe there will be little difficulty on the part of any adequately provided observer in verifying them. As some indication of the extent of my instrumental means I may mention that, under suitable circumstances, I have never failed to make out the *Pons Schröteri* and the nebulosity of the included part of the *Sinus Magnus*, since I first caught sight of it a few hours previous to the great earthquake on the night of Oct. 5, 1863. I have also, during the present season, repeatedly distinguished traces of the *Nebula Oblongata*. The *Lacus Lasselii* I find, however, very difficult; and it is certainly much less conspicuous than the dark opening, or "lake," to which I have directed attention, lying N.W. from the trapezium.

Hardwick Parsonage, Feb. 7, 1866.

P. S.—Having had another opportunity, on February 17, when the weather was favourable, of examining the region S. of the trapezium with a power of 450, I beg permission to send a supplementary diagram, in which it will be observed that an additional "cumulus" has been introduced on the line of the "frons," according to Sir John Herschel's nomenclature. The general appearance of this part was as though the "frons" and "rostrum" consisted of a row of contiguous masses, about six in number, of which only the three preceding the bright star 93 were sufficiently insulated to be distinguished sepa-

rately. The second and third of these, reckoning backwards from the S. point, formed an equilateral triangle with a cumulus in the interior.

The dark rift or channel was not very distinct on that evening; but the opening into which it leads was fairly visible. I doubted of its continuation beyond the opening through the nebulosity to the west.

Path of a Detonating Meteor. By A. S. Herschel, Esq.

Shooting-stars, it is well known, are so abundantly observed on certain nights of the year, that already, at the end of the last century, the term *meteorode* was applied on this account to the night of the 10th of August. It is suspected that large meteors also make their appearance on fixed nights of the year, although not with the same frequency or regularity as the more constant star-showers. Detonating meteors were observed on the 10th of February, 1772, by Brydone; on the 11th of February, 1850, by the present Astronomer Royal; and on the 9th of February, 1865, by a friend of the writer at Bangalore (S. India); all of which probably belonged to the same zone of meteors circulating round the Sun.

On the 21st of November, 1865, at $6^h\ 5^m$ G.M.T., a meteor about three times as bright as *Venus* is at its brightest, and having an apparent diameter of 8′ or 10′, was observed by Mr. Warren De La Rue near Cranford. The meteor rose from the eastern horizon, being surrounded at first, like a comet, by a parabola-shaped halo of light, reminding an observer of the zodiacal light, in its form, but rapidly rising and becoming brighter. When at an altitude of about 40° due east the meteor emerged like a fire-ball from a roman candle, of a blue colour, about a quarter of the apparent diameter of the full Moon in width, and drawing after it a trail of reddish-coloured sparks, $2\frac{1}{2}^\circ$ or 3° in length. As it passed overhead, the outpouring and falling behind of the matter forming the train became distinctly visible. At Wimbledon, near London, where the meteor was seen by Mr. F. C. Penrose, it vanished suddenly, or at any rate very rapidly, about 8° N.W. of α *Lyræ*. No streak appears to have been left, or, if conspicuous, it was hidden by a haze which generally overspread the sky. An account of the meteor having been published, and information from other observers regarding its appearance being requested by Mr. De La Rue, observations from places as far distant as Liverpool were received, as well as accounts from places more near to the mouth of the Thames, where the meteor at first made its appearance in the zenith. From a

comparison of these accounts it is possible to determine, at least approximately, within small limits of error, the real altitude, position, and velocity of the meteor.

The height of the meteor, at the moment of its first appearance, is determined from six independent descriptions, each of which, separately compared with Mr. De La Rue's original observation of the meteor at Cranford, furnishes an estimate of the real height of the meteor at its first appearance. The mean of all the separate heights is taken as the real height; and the average error from the mean of all the separate heights is seven British statute miles.

The height at disappearance is determined, in an exactly similar manner, from the same six accounts compared with the observation of the apparent place of disappearance of the meteor, as observed at Wimbledon by Mr. F. C. Penrose. The accuracy of the determination is about the same as in the former case, or about seven miles, either above or below the mean. When the nature of the phenomenon is considered, and particularly the difficulty of fixing the exact position of the apparent path of the meteor, as seen by the several observers, it is not by any means surprising that such differences should exist in the separate observations, but rather surprising that such good observations could be made.

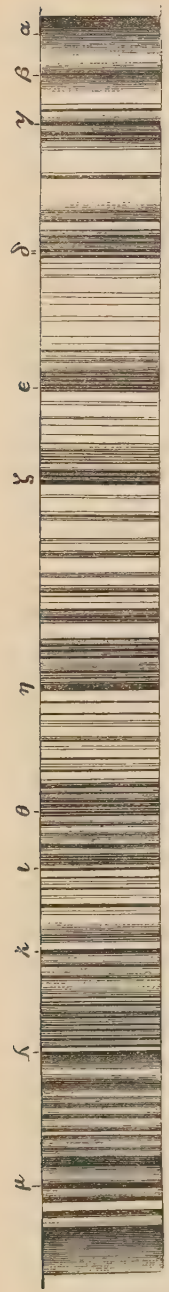
The meteor of the 21st of November, 1865, traversed the entire length of the valley of the Thames,—a distance of about seventy-five miles,— from forty-one miles above the Nore to twenty-seven miles above the Earth's surface in the neighbourhood of Henley-on-Thames. On the average of four separate accounts, estimated by different observers between four and ten seconds, the time taken by the meteor to travel the entire distance, about seventy-five miles, was six seconds and a half. On this estimate the velocity of the meteor, relatively to the earth's surface, was about eleven miles per second. The direction of the actual position of the meteor's flight was from a point in the neighbourhood of the constellation *Taurus*, between *Taurus* and the head of *Cetus*. The meteor of the 19th of November, 1861, about to be mentioned, was observed at Woodford as at first stationary for two seconds at a point in *Cetus*.

The distance of the meteor, at the moment of its disappearance, from Wimbledon, collectively determined from these accounts, is about thirty-six miles. At Wimbledon Mr. F. C. Penrose heard a loud report, like that of a cannon fired off at the distance of some miles, distinct enough to be heard very plainly by one other person at Wimbledon, about two minutes and twenty seconds after the meteor had disappeared. Sound, with its ordinary velocity of 1090 feet per second in common air, would take two minutes and fifty-four seconds to travel the entire distance of thirty-six miles from the point of the disappearance of the meteor to Wimbledon. Considering, as before,

the difficulty of fixing the exact position of the apparent path of the meteor, and hence the approximate nature of the real path concluded from the independent statements of the observers, the agreement of the calculated time with the time observed by Mr. Penrose, between the disappearance of the meteor and the occurrence of the sound, must be regarded as a near coincidence. There can be little doubt from this circumstance,— from the nature of the sound, the great apparent brightness of the fire-ball, and from its near approach to the earth,— that this meteor was really a detonating fire-ball.

Detonating meteors are described in the British Association Reports as having taken place in England, during the last five years, very nearly on the same date of the year as the meteor of the 21st of November, 1865. The first of these meteors exploded over Ipswich and Norwich, with three distinct reports, like heavy ordnance or distant thunder, on the evening of the 19th of November, 1861. The sound of the explosion of the second was heard at Hallaton, in Rutlandshire, on the 20th of November, 1864, like distant artillery, lasting several seconds. Finally, a detonating meteor of unusual size, described by Kepler in his *Ephemerides* (cited by Halley in the *Philosophical Transactions* for the year 1719, p. 978), which passed over Germany with a report like thunder, heard in Austria, on the 17th of November, 1623 (N.S.), may be thought, with great probability, to belong to the same zone of meteors encompassing the Sun. The sidereal year exceeding the length of the tropical year by almost exactly one day in seventy years, the position of the Earth in its orbit at the time of the occurrence of Kepler's meteor (nearly two centuries and a half ago) would coincide with the Earth's place in its orbit, at the present time, on the 21st of November; exactly the date when Mr. Warren De La Rue's meteor was observed.

The epochs of the 9th–11th of February, and the 19th–21st of November are, therefore, dates deserving special attention, partly with a view of determining for the future the directions of the detonating meteors, and partly as showing, by their frequent return within very narrow limits of time about those dates, that aërolitic meteors, like the acknowledged star-showers of August and November, revolved in fixed orbits round the Sun.



Spectrum of α Orionis. By the Rev. Father Secchi.

(*Extract from a Letter addressed to the Secretary.*)

I take the liberty of sending a sketch of a drawing of the spectrum of *α Orionis*. It has been made with an apparatus of M. Merz, of Munich, combined with a prism of Amici, made by Hoffman. The details are strong and wonderful, although the prism is very small. I hope to be able to make new observations with a larger prism and yet greater light.

As a point of reference, I mention that the line δ is that of sodium, and η that of magnesium.

There is some considerable difference in the position of some nebulous bands, as compared with the figure of Mr. Huggins'. I am sure there is no fault in the measures. Is there any real change?

The spectrum of *Sirius* is seen with this instrument most admirably. It appears all striated by narrow equal bands, like the spectrum of *Sulphur* in the plate of M. Plücker, given in the *Philosophical Transactions* for the year 1865.

I suspected some illusion in these equal bands, and I looked to *Rigel*, but I found that there also are the bands, but much narrower, almost as the spectrum of nitrogen in the same plate; $4\frac{1}{2}$ bands of *Sirius* are equivalent to $6\frac{1}{2}$ of *Rigel*.

I send you also a photograph of a solar spot observed on the 16th January last. It will show how the details and veils are seen with the polarising eye-pieces. The red colour of some veils vanishes with common eye-pieces, even if diagonal.

Rome, Feb. 10, 1866.

Note on the Spectrum of the Variable Star α Orionis, with some Remarks on the Letter of the Rev. Father Secchi. By William Huggins, F.R.S., and W. A. Miller, M.D., LL.D.

P. Secchi, in consequence, as he states, of "some considerable difference in the position of some nebulous bands" in his diagram from the figure which accompanies our paper, "On the Spectra of some of the Fixed Stars,"* asks, "Is there any real change?"

A real change in the spectrum of a star, since it implies either a change in the chemical constitution of the star, or important physical changes which effect an alteration in the investing atmosphere of vapours, to the absorptive action of which the lines in the spectrum are due, should not be assumed, except as the result of observations made with the extreme care that so delicate an investigation requires. We do not think that it is possible to determine a question of so much importance to cosmical science from a comparison of P. Secchi's diagram with our own, for the reasons which follow.

P. Secchi gives no information as to how many of the lines in his diagram have been laid down from actual measurement. Into our diagram those lines only were admitted of which the measures are contained in a table which accompanies the diagram. Our figure, therefore, is not intended as a *full* representation of the spectrum *as it appeared* in the instrument,—for a number of lines, much greater than the eighty measured lines given by us, might easily have been inserted, if we had not thought that the trustworthiness of the diagram would have been impaired by the addition of lines of which the positions were only estimated. Our measures were afterwards checked and verified by the simultaneous comparison, in the instrument, of the star spectrum with the bright lines of sixteen elementary substances. The positions of several of the brightest lines of each of these elements are given in our diagram relatively to the measured lines of the star.

Again, since the relative proportions of the different parts of the spectrum are affected by the quality of the glass of which the prisms are made, and also by the positions of the prisms in relation to the light incident upon them, the diagram of *α Orionis* should be accompanied by measures with the same instrument of some well-known lines of reference, as the principal lines of Fraunhofer. P. Secchi says that "the line δ is that of sodium, and η that of magnesium," but with these positions the colours placed beneath the spectrum are not in agreement. In that case, the part of the spectrum under which "azzuro" is written, would be about E of the solar

* *Phil. Trans.* 1864, p. 413.

spectrum, and on the same scale, the part of the spectrum above "violetto," would be a little more refrangible than Fraunhofer's F.

It may be well to mention here that for the measurement of stellar spectra a very narrow slit must be employed. For since it is not possible by the clock-motion of an equatoreal instrument to maintain the image of a star in an invariable position within the jaws of a rather wide slit, the measures will not be satisfactory, unless the width of the slit is less than that of the image of the star made linear by the cylindrical lens, and the passage of the stellar image before the slit is seen to make no sensible alteration in the position of the lines of the spectrum.

The spectroscope, with which our measures were made, has been preserved with its adjustments unaltered. With this instrument, on the 6th and 12th of March, we measured the more refrangible terminations of the shaded groups of lines which, in our diagram and table of measures, are denoted by the numbers 840, 920, 1169·5, 1300·5, 1420. The identity of our recent measures with those which we made during the first three months of 1864, proves that no change has taken place in the positions of these groups during the last two years. The "shading as of fine lines" is still seen, as in our diagram, on *both sides* of the lines coincident with those of sodium.

An important change has, however, been recently observed by us in the spectrum of this star, though not a change in the *position* of any of the groups, such as has been suggested by P. Secchi.

α *Orionis* is a variable star of great irregularity, both of period and of extent of change of brightness.* Now we have recently found that the group of lines and "shading as if of fine lines" terminated at its more refrangible end by the strong line, No. 1069·5 in our diagram, is not at present visible in the spectrum of the star. The absence of this group is of great interest in connexion with the variability of the star's light, especially as the time of the disappearance of this group coincides with the epoch of the maximum brilliancy of the star.

Since we might be biassed in our estimation of the present degree of brilliancy of the star, by our knowledge of this peculiar change in its spectrum, one of us wrote to Mr. Baxen-

* Sir John Herschel writes:—"The variations of α *Orionis* which were most striking and unequivocal, in the years 1836–40, within the years since elapsed, became much less conspicuous. In Jan. 1849, they had recommenced; and on Dec. 5, 1852, Mr. Fletcher observed α *Orionis* brighter than *Capella*, and actually the largest star in the northern hemisphere; *Outlines of Astronomy*, p. 602. Until the present time, the changes of this star appear to have been again inconsiderable. See Schmidt, *Ast. Nach.* No. 1570."

dell, whose successful prosecution of this branch of astronomy is so well known, requesting him to state in what phase of its period of variation *α Orionis* is at the present time. His reply is as follows:—"March 9, 1866. The variable *α Orionis* is irregular both in the extent of its variation, and in the duration of its period. It has been increasing in brightness since the end of December, and is now at, or very near, a maximum. I have often thought that its light was at times variable in colour as well as intensity, being sometimes very perceptibly more ruddy than at others. It is, however, a difficult star to observe, owing to its brilliancy and its ruddy colour, which is differently acted upon by haze or moonlight to that of a white star."

The variation in colour, so well described by Mr. Baxendell, corresponds exactly to the change in the colour of the star, which would be produced by the absence or presence of the group of lines of which we are speaking, since the position of this group in the spectrum is about the boundary of the "orange" towards the "yellow."

We have been for some time occupied with observations of other variable stars, which we believe may give to us new information of the probable mode in which, in some stars at least, the periodic alteration of light of these remarkable objects is brought about. We should, therefore, have much preferred reserving, for the present, our observations of the remarkable fading out of this group of lines in the spectrum of *α Orionis*. We may mention here that the fine lines in the spectra of *Sirius* and *Rigel*, referred to by P. Secchi, were described by us in our paper read before the Royal Society in May, 1864, in the following words :—

"*Sirius.* The spectrum of this brilliant star is very intense, but owing to its low altitude, even when most favourably situated, the observation of the finer lines is rendered very difficult by the motion of the earth's atmosphere. . . . . Three, if not four, elementary bodies have been found to furnish spectra in which lines coincide with those of *Sirius*, viz. sodium, magnesium, hydrogen, and probably iron. . . . . The whole spectrum of *Sirius* is crossed by a very large number of faint and fine lines. . . . . It is worthy of notice that in the case of *Sirius*, and a large number of the white stars, at the same time that the hydrogen lines are abnormally strong as compared with the solar spectrum, all the metallic lines are remarkably faint."

"*Rigel.* A spectrum full of fine lines."

The appearance of these fine lines is not such as to suggest to us any resemblance to those spectra of sulphur and nitrogen which are referred to by P. Secchi.

* *Phil. Trans.* 1864, pp. 427, 428, 429.

Occultation of 130 *Tauri.* By C. G. Talmage, Esq.

1866, February 23. Occultation of 130 *Tauri.*

G.M.T. of Disappearance,

$= 5^h\ 30^m\ 51^s{\cdot}73.$

Star rather faint.

G.M.T. of Reappearance,

$= 6^h\ 23^m\ 54^s{\cdot}26.$

Came out exceedingly bright and sharp for a 6th mag. star; time exact.

Note on the Companion to Antares. By D. A. Freeman, Esq.

I beg permission to say that, on turning my Cooke's $4\frac{1}{2}$-in. refractor on *Antares* an hour before sunrise, on the 22d inst., the atmosphere being in an exceptionally fine condition, the small star was very clearly visible, and, at times, quite free from the light round the large star. The colour of the small star appeared to be green; and, with a negative eye-piece magnifying 180, I kept it steadily in view till the Sun had appeared upon the horizon.

This star being clearly visible with a small object-glass of $4\frac{1}{2}$ inches makes it a matter of surprise that it should have been discovered but recently, only though, from its low declination, it is probably more difficult to see it well in England than in this latitude.

Mentone, Alpes Maritimes, Jan. 24, 1866.

Ephemeris of Iris for the Opposition of 1866.
By F. Brunnow.

The following Ephemeris for the opposition of *Iris* has been calculated by me from my Tables. In constructing these, the perturbations of the first order produced by *Jupiter*, *Saturn*, and *Mars*, as well as the terms depending on the square of the mass of *Jupiter*, have been taken into account. As the elliptical elements of the orbit are pretty accurately known, I expect that the error of the Ephemeris will be small, and that the tables will give the planet's place with sufficient accuracy for a long time to come. They represent six normal places distributed over a space of eighteen years very satisfactorily, the errors being as follows: —

		Δ cos α.	Δ δ.
		s	″
1847	Aug. 20·0	+0·27	+1·98
1851	Oct. 3·5	−0·25	−1·71
1855	Dec. 14·5	+0·05	+0·51
1860	Feb. 8·5	+0·06	+0·85
1862	Sept. 11·5	+0·12	+1·33
1865	June 10·5	−0·25	+0·71

Ephemeris for Berlin Mean Midnight.

		Hourly Motion.			Hourly Motion.	Log Distance.
	h m s		o ′ ″			
Nov. 18	4 59 38·24	−2·052	+25 25 59·5	−13·82	9·95950
19	58 48·18	2·116	25 20 24·0	14·12	9·95847
20	57 56·66	2·175	25 14 41·5	14·41	9·95755
21	57 3·77	2·230	25 8 52·3	14·69	9·95674
22	56 9·62	2·280	25 2 56·5	14·96	9·95604
23	55 15·30	2·327	24 56 54·4	15·22	9·95544
24	54 17·92	2·369	24 50 46·2	15·46	9·95491
25	53 20·57	2·407	24 44 32·2	15·70	9·95457
26	52 22·36	2·441	24 38 12·8	15·92	9·95430
27	51 23·40	2·469	24 31 48·2	16·12	9·95415
28	50 23·84	2·491	24 25 18·9	16·31	9·95410
29	49 23·82	2·508	24 18 45·1	16·49	9·95416
30	48 23·44	2·521	24 12 7·4	16·64	9·95434
Dec. 1	47 22·82	2·527	24 5 26·2	16·78	9·95463
2	46 22·11	2·529	23 58 41·8	16·91	9·95504
3	45 21·44	2·525	23 51 54·7	17·01	9·95556
4	44 20·93	2·515	23 45 5·4	17·09	9·95620
5	43 20·70	2·500	23 38 14·3	17·15	9·95695
6	42 20·90	2·480	23 31 22·1	17·19	9·95782
7	41 21·64	2·455	23 24 29·3	17·20	9·95881
8	40 23·07	2·424	23 17 36·3	17·20	9·95990
9	39 25·29	2·388	23 10 43·8	17·17	9·96110
10	38 28·42	2·348	23 3 52·3	17·11	9·96241
11	37 32·58	2·303	22 57 2·3	17·04	9·96384
12	36 37·88	2·253	22 50 14·3	16·95	9·96537
13	35 44·41	2·200	22 43 28·7	16·84	9·96701
14	34 52·29	2·142	22 36 46·2	16·70	9·96875
15	34 1·61	2·080	22 30 7·2	16·54	9·97059
16	33 12·45	2·015	22 23 32·2	16·36	9·97253
17	32 24·88	1·947	22 17 1·7	16·17	9·97456
18	31 38·97	1·876	22 10 36·2	15·95	9·97669
19	30 54·81	1·802	22 4 16·1	15·71	9·97890
20	4 30 12·48	−1·725	+21 58 2·0	−15·45	9·98121

Dunsink, Observatory of Trinity College, Dublin.

ERRATA.

Page 144, line 5 from bottom, *for* 190 H. II., *read* 199 H. II.

— — — 9 — *for* 51 H. I., *read* 52 H. I.

— — — 15 — *for* 38 H. IV., *read* 38 H. VI.

— — — 8 — *omit* 4627 192 H. I.

— 150, — 12, *for* Arlem, *read* Anclam, Pomerania.

CONTENTS.

	Page
Fellows elected	103
Investigations on Airy's Double-Image Micrometer, by Prof. Kaiser	ib.
Additions to the Investigations on Cometary Systems, by M. Hoek	204
Notice of the Great Nebula in *Orion*, by the Rev. T. W. Webb ..	208
Path of a Detonating Meteor, by Mr. Herschel	211
Spectrum of α *Orionis*, by the Rev. Father Secchi	214
Note on the Spectrum of the Variable Star α *Orionis*, with some Remarks on the Letter of the Rev. Father Secchi, by Mr. Huggins and Dr. Miller	215
Occultation of 130 *Tauri*, by Mr. Talmage	218
Note on the Companion to *Antares*, by Mr. Freeman	ib.
Ephemeris of *Iris* for the Opposition 1866, by M. Brunnow	ib.

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